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Biological
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Serials

VOL. IV. No. 4. FEBRUARY, 1911.

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TRANSACTIONS

OF THE

ROYAL SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

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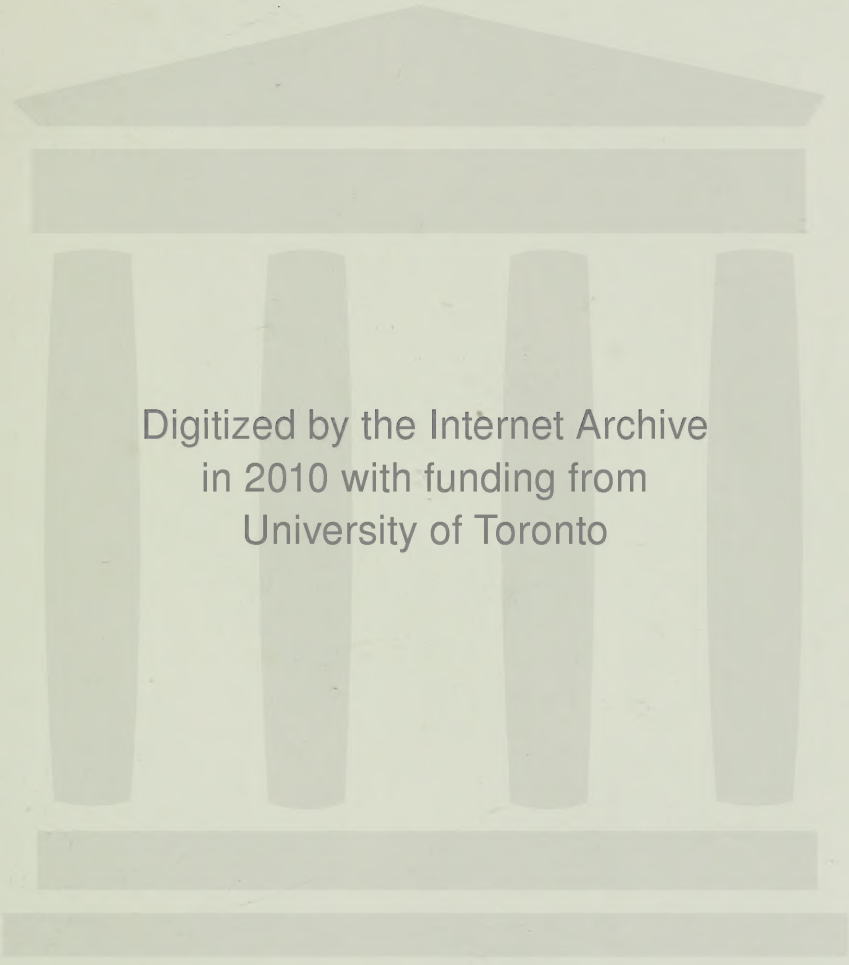

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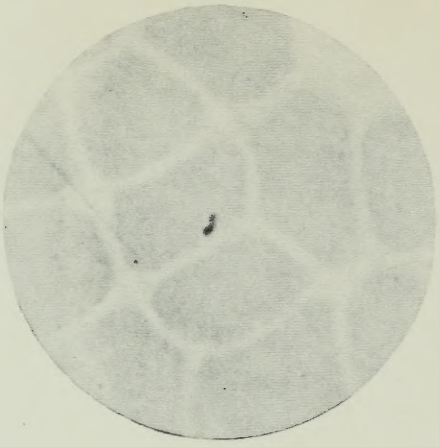

Fig 1.—Double Chromatin—body in blood corpuscle.

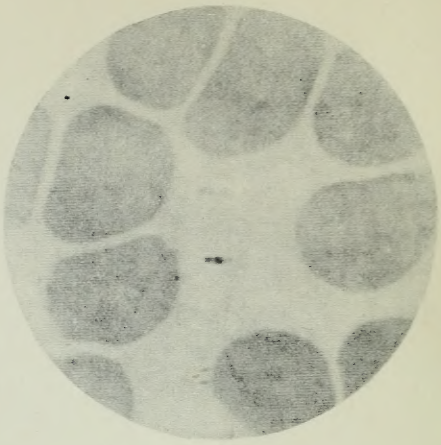

Fig. 2.—Free body in blood.

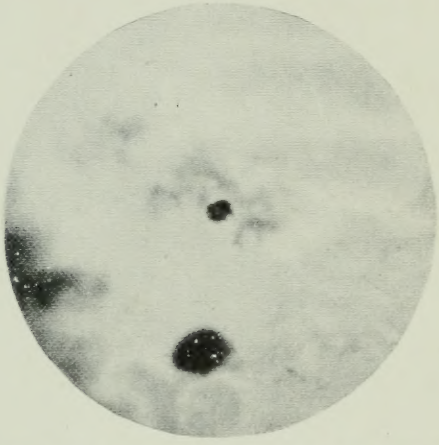

Fig. 3.—Ring form—multiple chromatin—bodies in capillary in kidney.

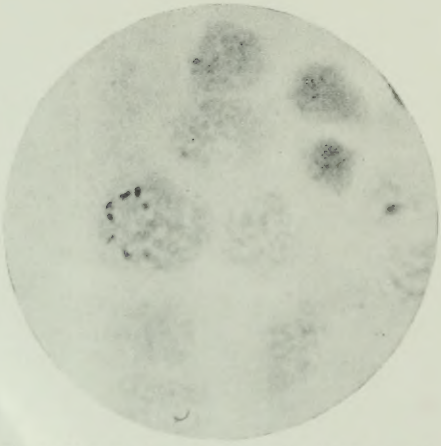

Fig. 4.—Multiple chromatin—bodies in blood corpuscle in kidney.

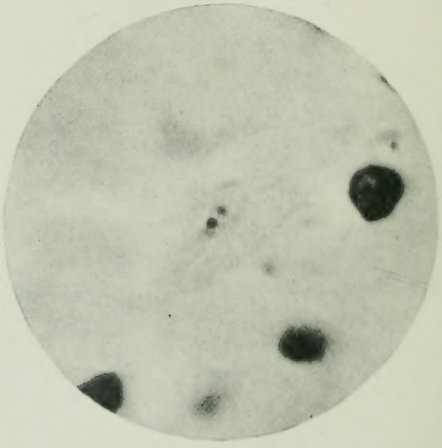

Fig. 5.—Bodies in tissue of spleen.

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OF THE

SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

FEBRUARY, 1911.

VOLUME IV. No. 4.

Proceedings of a Meeting of the Society on Friday, January 20th, 1911.

PROFESSOR RONALD ROSS, C.B., F.R.S., *President*, in the Chair.

The PRESIDENT, before calling upon Dr. DURHAM to resume the discussion on Sir RUBERT BOYCE'S paper, stated that the Council had considered the desirability of bringing forward a resolution in order to secure greater definiteness in the discussion. This was not considered desirable, but it was hoped that those taking part in the discussion would direct their remarks to the conclusions which Sir RUBERT BOYCE had submitted to the Society in the following terms:—"Yellow fever is endemic and common in West Africa, and has occurred in all its various forms from the *embarras gastrique* to black vomit. There is but one remedy, and that is stegomyia destruction; and there is but one result to expect, namely, a most marked increase in the health and prosperity of West Africa."

He also expressed regret that Professor OSLER, who had intended to take part in the discussion, was prevented from being present, and later in the meeting that Professor NUTTALL, who desired to support the conclusions of Sir RUBERT BOYCE'S paper, was unable to stay sufficiently long to contribute to the discussion.

The discussion on Sir RUBERT BOYCE'S paper on "The Distribution and Prevalence of Yellow Fever in West Africa," which had been read at the meeting in December, 1910, was then resumed.

Dr. HERBERT E. DURHAM said: In the first place, I must express surprise that Sir RUBERT'S proposition has been called "new." I was under the impression that ten years ago the state of affairs he describes was well known; it is regrettable that active measures have not yet been undertaken.

Administration.—A prime need, if yellow fever is to be banished from West Africa, and our other infected colonies, is that we should have strong and energetic, as well as brave, men as principals at home and abroad. Where doubt may exist, they should appreciate that the benefit of the doubt should be given to the newcomer, and non-immune, rather than to the yellow fever germ. Those at the Colonial Office should be kept in touch with, and have knowledge of, the local affairs and conditions of the various Crown Colonies; this result might be enhanced by means of surprise visitations—a sort of quasi-espionage. The empire needs a properly qualified Minister of Public Health, whose sphere of action and influence would not be limited to the home country. The health of this country is at present cared for merely as one of the motley collection of things, which are administered by the Local Government Board, under the ægis of men, who are not expert hygienists. An allocation of a considerable proportion of the stipend of the Local Government Board, and smaller proportions of those of the Board of Agriculture and Colonial Office, would serve to institute the office of one of the most important of our Ministers—of home and empire—of the health of man and beast.

Factors contributing to Endemicity of Yellow Fever.—Except for the presence of typical cases of yellow fever we have no criterion of its existence; the germ is not recognisable—we have no method of serodiagnosis or other clinical test. For typical cases we have to rely on the chance infection of some unfortunate newcomer.

I gathered from the late Major REED, that he was inclined to lay considerable stress on the longevity of the infected mosquito, together with the importation of newcomers, as the chief means of keeping the fever alive. I think that more than this is necessary to ensure a constant supply of actively infected mosquitoes. If it be granted that the advent of newcomers and primary attacks in children of residents or natives are insufficient, we have as other possibilities, (1) the persistence of the virus in man after a single infection, and, (2) through successive

inoculations of one and the same individual (so-called "immune"). On the whole, the evidence is against a persistence of the virus in the blood of one individual after a single infection; the lesson from Havana is distinctly in this direction, but it should be remembered that a sort of commensalism, such as we see in some other diseases, is not an impossibility. The re-infection of so-called "immunes" is, I believe, an important factor in the establishment of endemicity; here much is bound up with the question of immunity, its meaning and its extent.

Immunity.—Much darkness appears still to prevail in our conceptions of this attribute of infective disorders, which in nature never approaches near the degree of comparative absolutism that can be obtained in laboratories. It used to be stated, and I daresay is still stated in text books, that each specific fever gives protection against further attacks. This is generally true, if we adhere to the old conception of a fever simply as a clinical picture; but that conception has, or ought to have, passed away and given place to the supreme criterion of the presence of the essential factor—the *causa morbi vera.* Experiments have shown that there is no relationship between the amounts of antitoxic and of microbicidal matters that are engendered in the system during an infection. I take it that in yellow fever the antitoxic is the more prominently developed, and hence the microbe can live or multiply in the system of a so-called "immune" sufficiently to serve to infect mosquitoes, though its normal poison is largely, if not completely, annulled, whether by antitoxic destruction or by acquired toxin-tolerance. Hence an immune may show but slight fever, and his kidneys and liver may well-nigh escape altogether.

The protection given by a single attack of yellow fever is well known to be finite; so far as I was able to make out from records, it lasts about four years. That is to say, absence from an endemic area for about that time renders a person liable to a *typical* attack on re-exposure. *This means surely that permanent residents must be constantly renewing, and adding to their degree of immunity,* in as much as they, proverbially, escape further typical attacks. In my report from Pará I gave special instances of severe and light cases, which were probably of the nature of yellow fever in "immunes"; and I may add that slight febrile attacks both in children and adults were quite common in the city, which was apparantly free from malarial infection except at its outskirts. Myself

and three other "immunes" had a few days' indisposition shortly after a severe and fatal case of yellow fever in the next house but one in the same street; this, I believe, was simply an immunising dose. Such cases naturally give no indication of their real nature, and exactly the same may be said of the transient cases of fever in "non-immunes"; one meets with many persons who "never have had yellow fever" and some who "do not believe in it." I take it that often, if not always, this failure to be attacked is the manifestation of the nature of the passing febrile indisposition, to which some persons will confess to having had. (Dr. TULLOCH mentioned that he had apparently escaped "yellow fever," although he must often have been exposed to infection). It may be laid down dogmatically that all cases of fever, such as "febricula," "acclimatisation," "catarrhal" fever, etc. *(febre do acclimitisão, da transpiracão*, etc.), should be notified as suspect. The problem of the mild case, its difficulty of recognition, and its danger as a focus for radiation, is not one that pertains to yellow fever alone. It is a problem that is here with us in our domestic endemic fevers, and their occasional epidemic exacerbations. If all cases gave the true-to-type clinical picture the task of stamping out would be easy, for then the immune carrier would cease to exist. I need hardly call attention to the disastrous results of errors in diagnosis in atypical small-pox, the acceptance of certain forms of "croup" as entities apart from diphtheria, and the beneficent results of the recognition of the diphtheria bacillus in mild sore throats, or of typhoid by sero-diagnosis in otherwise innominate febrile cases.

Bilious remittent fever, that is to say, the *febre remittente biliosa*, or *dos paizes quentes* of the Brazilian, needs but few words. The few cases, which I saw thus diagnosed in Parà, showed no distinct point of difference from yellow fever, nor could I get a satisfactory reason for the diagnosis from the physician in charge. The prime need of the diagnosis appears to be that the disease occurs in natives. In Rio de Janeiro (TORRES HOMEM) the yellow fever and bilious remittent fever seasons coincide, and the distribution of yellow fever within the city limits maps out the distribution of *remittente biliosa*. In a word, the two diseases appear hand in hand, and GUITERAS has said that they have died out side by side in the Southern States of North America. Whatever other fevers may be included under the term "bilious remittent," it

appears obvious that one manifestation of yellow fever gives the clinical picture of bilious remittent—chiefly when it occurs in a known "immune." On the West Coast of Africa both diseases co-exist, so we are told; let the one be eliminated, and let us see what happens to the other! In the discussion I find no record of the pathological anatomy—for instance, the histology of the liver, kidney or spleen—in the accounts of bilious remittent fever; surely this is a gap which requires attention. I found the destruction of the adenoid sheaths of the arterioles in the spleen a remarkable feature in the anatomy of yellow fever, and I would call attention to the swelling and inflammation of the superficial lymphatic glands (axillary, cervical, etc.). *Febre remittente biliosa* appears, just like the *Fiebre de Borras*, of Cuba, to be merely a fraudulent attempt to delude the newcomer, or intending newcomer, and I would suggest that the diagnosis "bilious remittent" should only be accepted as a clinical entity when, in the same surroundings, there is a considerable number of non-immunes, all of whom fail absolutely to show any sign of being attacked by more or less typical yellow fever.

The part played by children.—The following quotation from TORRES HOMEM *(Estudo clinico sobre as Febres do Rio de Janeiro, 1877)* is of interest from several points of view:—"In epidemics which I have observed, it is not only the foreigners that pay a heavy penalty; the Brazilians who live in the Southern States (S. Paulo, Rio Grande do Sul, etc.) when they arrive (at Rio de Janeiro) are almost infallibly victims of the disease, chiefly when they go to live in the heart of the city. . . . . In the epidemic of 1873, when the fever also attacked acclimatised persons, it preferred children from two to ten years old; these yielded the greater number of deaths among those who were not foreigners; in February alone, 75 children died from this cause." Children have been shown to be important carriers of malaria abroad; they are the chief means of propagating scarlatina, morbilli, etc., at home. They are probably of very considerable importance in yellow fever. But very careful attention must be paid to them, for, as SODRÈ and COUTO have shown, the disease has a totally different "clinical picture" to that in the adult—the rarity of *vomito preto*, kidney trouble, jaundice, etc., and the prominence of meningeal symptoms. Such cases I have seen in Parà.

Supply of Stegomyia mosquitoes.—Lastly, there must be an abundant

supply of suitable mosquitoes for the fever to be propagated. There must be enough to ensure that some succeed in sucking up the virus of the fever as they feed. Without the mosquito the disease must die out.

Summary.—In summing up the points I have raised, I will again urge the need of a strong, educated and brave administration; the need of paying minute attention to every slight case of fever or indisposition, whether in an immune or a non-immune; the importance of recognising the fever in the form in which children show it; the need of effective destruction of stegomyia mosquitoes; and of research into the true nature of the (perhaps mostly so-called) "bilious remittent fever" which occurs in yellow fever countries.

Sir PATRICK MANSON: I have great diffidence in taking part in this discussion, mainly because I have had no personal experience of yellow fever, and it is skating on thin ice for a man to talk about a disease he has never seen. As I have said I have not yet seen yellow fever, but I have seen cases of bilious remittent fever, and indeed have suffered from it myself in China, in which country there was no suspicion of yellow fever ever having been present. Now, in this case, if bilious remittent fever can occur in a non-endemic area, its occurrence in an endemic area of yellow fever is no proof that it is yellow fever.

I notice, Mr. PRESIDENT, that you have requested us to confine the discussion to-night to Sir RUBERT BOYCE'S conclusions. These are, as I understand them, that the most important and best way of fighting yellow fever is to attempt the extermination of the stegomyia mosquito. In that view, I am quite sure all members of the Society concur; certainly I believe it is the right way to tackle the disease. But it is Sir RUBERT BOYCE'S contention, so far as I recollect, that the disease is endemic in West Africa. The expression "endemic" I understand to mean, that yellow fever occurs in all the principal towns of West Africa, that practically it is universal there; at all events in those towns. If Sir RUBERT BOYCE only means that the disease occurs year after year in West Africa, I agree with him; but with the view that it is *universally* endemic in West Africa, I distinctly disagree. Sir RUBERT BOYCE bases his conclusions on the hypothesis—and please remark, it is only hypothesis and not established fact—that yellow fever is particularly mild in children, that children becoming infected acquire immunity from

attacks of serious yellow fever, and that the immunity is maintained (as Dr. DURHAM suggests) by a reinoculation about every four years. This is the hypothesis of SIMOND and MARCHOUX, and I admit it is very generally adopted. But it is only hypothesis; and I think to contemplate serious action on the assumed correctness of an unproved hypothesis is a most dangerous proceeding. More than once we have committed serious errors by acting on unproved hypothesis. For example, it was an erroneous idea that cholera was an air-borne disease. Enormous expenditure of life and treasure was the result of this mistaken hypothesis: now we know that cholera is not an air-borne but a water-borne disease. Until hypotheses are proved up to the hilt I deprecate anything like popularising the idea that they are facts.

I could suggest not one hypothesis only, but several hypotheses, which, to my mind, could explain that immunity of native races of which Professor BOYCE makes so important a basis for his argument. For example, yellow fever is conveyed by stegomyia mosquito. Stegomyia mosquito is, I may say, omnivorous in its habits: that is, it bites beasts as well as man. Therefore, if a stegomyia mosquito bites a man with yellow fever and afterwards a beast, when it bites that beast it will implant the virus of yellow fever. It is no mere speculation that the virus of yellow fever will multiply in some of the lower animals. THOMAS has shown that yellow fever can be conveyed to the chimpanzee by inoculation. If this can be done with one animal why not with another. Therefore there is some reason to justify the speculation that the virus may be conveyed to man from lower animals. Now if this is so, and stegomyia bites an animal having the virus and afterwards bites a man, the man will in all probability receive the virus; it will multiply in him and he will get yellow fever. The fever germ may be deprived of its virulent characteristics by its progress through the lower animal, as in the case of the variola germ by passage through the cow. Let us suppose that this is the case. You get a mild strain of yellow fever virus, which can be transmitted indefinitely by stegomyia, just as vaccinia can be transmitted by the vaccinator's lancet. This process will go on through the community, and gradually we will get a community thoroughly and harmlessly immunised to every form of yellow fever. This is by no means proved; but it is as just as probable as the hypothesis of Professor BOYCE, and I suggest it for your con-

sideration as a reason for—at all events—modifying the very definite conclusion that he wishes us to accept.

With reference to the attitude of those who hold administrative posts, I should like to say that I have much sympathy with the responsible official. Sometimes a medical man brings forward an erroneous though plausible idea which misleads the official on the spot into acts for which he is held responsible, and which may cost the country much before the error is detected. He is therefore chary in accepting all medical advice without adequate proof of its soundness, and, in my opinion, very properly so. Returning to the hypothesis of Professor BOYCE: If you apply his theory in practice, you will find that one official would act absolutely differently to another, who might accept the equally plausible one I have offered. Suppose an official had Professor BOYCE's idea brought to his notice, he might act on it if he believed in it. Suppose this responsible official in some West African town is approached on the subject of yellow fever and he is told that yellow fever is endemic in a neighbouring town, and it is suggested to him that he should put on a quarantine against ships coming from that neighbouring town; he would say, "No, why punish my particular colony by putting on a quarantine against a disease which is always present in their midst, but from which the majority of my people are immune?" He will say that one or two cases more or less will not aggravate the risk. But suppose he believes in the hypothesis that his people are vaccinated by stegomyia with a mild form of disease, such as my hypothesis predicates, he will certainly adopt quarantine, because he will say, "There are certain foreigners and natives here who have not as yet had the chance of being rendered immune to the disease, by being vaccinated against it, and therefore why expose them to an unnecessary and avoidable risk?"

These are some of the ideas that have occurred to me. I think Professor BOYCE has done the profession and the cause of public health an important service in bringing this subject forward so prominently, and undoubtedly this discussion and its publication will do a great deal to promote the consideration of this subject and the prevention of yellow fever in West Africa. At the same time I should like to see a little caution observed before adopting and acting on purely hypothetical views as to the character, nature and cause of the endemicity of this disease.

Dr. J. W. W. STEPHENS: I have little to say, having had no actual experience of yellow fever, but I have studied the views put forward by Sir RUBERT BOYCE at the last meeting, and, as far as I am able, have considered them in relation to the published facts and history of yellow fever, and I am forced to the conclusion that the evidence before us shews clearly that yellow fever is endemic in West Africa. I think, however, that a good deal depends upon what is meant by the word "endemic." Some seem to have expressed the view that "endemic" means the prevalence of yellow fever in every large town in West Africa, and that cases should consequently be occurring in these towns. But I think there is another view of endemicity, and that is, that cases of yellow fever smoulder, so to speak, perhaps widely scattered, forming a different category of endemicity. It is, I think, very difficult to define the extent of the endemicity of yellow fever, but that there is this endemicity, in the form I have indicated, I believe.

We have in yellow fever no such objective test as we have in malaria with the spleen or, more especially, blood examination. In the absence of these two tests (in the case of endemic malaria there are often no ordinary clinical signs) whole districts in West Africa would erroneously be returned as free from malaria, and we should find it very difficult to explain cases of malaria arising among Europeans. This, indeed, was the case until it was shown that the native children formed a reservoir of malaria. This being so, until we get these objective tests I do not see how, with our present knowledge, we shall be able to show to what extent yellow fever is endemic in West Africa.

If I have rightly understood Sir PATRICK MANSON, he has rather shifted the focus of endemicity, and has converted yellow fever into an epizootic disease, but I cannot see that this helps us very much in the consideration of this question of endemicity.

Dr. G. C. WALKER: I was at Cape Coast Castle in 1902, at the time of the outbreak there, and the cases I saw then were of just the same type as those I was later called upon to see by Dr. RALPH at Sekondi in May, 1910, and an examination of these cases showed them to be of exactly the same type as those seen in the previous epidemic of 1902.

To my mind, I think a great deal of yellow fever could be checked if

the low scrub around the towns were cut down. Wherever I went I found any amount of stegomyia in this low scrub.

Another objectionable feature calling for attention was the presence right in the heart of these towns of German traders, living in houses with numbers of natives sleeping underneath the houses, under the most filthy conditions, in spite of all we could teach them about keeping their places clean.

With regard to the question of immunity which has been mentioned, I do not know anything about the immunity of children, but the first case in the last epidemic to which I have referred was that of a woman. She recovered, and her husband was the second case to be attacked, and he died about thirty-six hours after the commencement of the attack.

In the case of yellow fever in natives, I must admit I had never seen a case that I could call yellow fever until the last outbreak, but all medical men agree that yellow fever does occur amongst natives.

With regard to the difference between yellow fever and bilious remittent fever, I am of opinion that, clinically, they are as distinct from one another as scarlet fever is from small-pox. Nevertheless, it is common knowledge that the diagnoses of these latter diseases are often confused. As to the endemicity of yellow fever, one must admit that small-pox is endemic in England, although there are huge communities in which small-pox does not occur. But, suppose you bring in a non-immune foreigner, he gets it. Similarly, Europeans go out to West Africa. You do not say that everybody has yellow fever, and yet the European is suddenly stricken with this disease. Where does he get it? In the outbreaks at which I was present, we tried to trace the source of the disease, but without success. The fact remains, however, that the disease is there.

Dr. Prout [contribution read by the Secretary in his absence]: Although I am unfortunately unable to be present at the adjourned discussion on yellow fever in West Africa, yet I should like to make a few remarks, based on my long experience in that part of the world. I have read Professor Boyce's paper with great interest, and I may add that I have had the very great advantage of discussing many points personally with him, so that the following criticisms may be regarded as indicating the difficulties which have occurred to me in the

way of accepting in its entirety his theory of the endemic prevalence of yellow fever in West Africa, and its existence in a hitherto unrecognised form among the natives of West Africa, for that, of course, must be the essential factor in its continuance if this theory is accepted. Much depends upon what is meant by endemicity. If it is understood that yellow fever is endemic on the West Coast of Africa in the same sense as small-pox is, that is, that it exists at one or several spots, that it dies out occasionally, breaks out elsewhere, and so on, I have no hesitation whatever in accepting it; but if by endemicity is meant that at every spot, all the year round, wherever the stegomyia is found, yellow fever exists in some form or other, generally quiescent, occasionally epidemic, I must confess that, so far as I have gone into the matter, Sir RUBERT has not yet succeeded in convincing me, though I preserve an open mind.

And we must consider carefully what the conditions of population on the West Coast are. The Europeans are scattered in small communities at the various government and trading stations in numbers varying from two or three upwards. In only a few do they number twenty, while in a very limited number indeed do the numbers reach one or two hundred. This is the case only at the headquarters at Bathurst, Freetown, Accra, Sekondi, the gold mines and Lagos, so far as the English colonies are concerned. But there is one factor, and it is a very important one from the point of view of yellow fever. The European population is a moving one, and it is constantly being added to by new arrivals. These number hundreds every year, so that there is a constant supply of non-immunes. I wish to emphasise this fact, because it is a constant factor: the Europeans in the past were always changing, and do so at the present time.

But there is one place on the West Coast where this condition is not so marked, and that is the French colony of Senegal. The white population there is much greater than elsewhere, it is much more permanent, French officials and traders stay longer periods, they take their wives and families with them, there are white regiments there; so that the conditions are very considerably different from those which obtain in the English colonies. And it is just in this French colony that the prevalence of yellow fever has been most marked of late years, and in which there have been fewest gaps; and Senegal is the *only*

colony on the West Coast which is in regular communication with South America. In Sierra Leone, on the other hand, the reverse has occurred. Of late years the gaps have been considerable, and the years in which yellow fever was most prevalent were those in which there were white regiments continuously resident.

Now, as regards native populations. In the large coast towns there is a large and constant resident native population, but in addition, at Freetown and Lagos, say, there is a very large and frequently changing, floating population. It is constantly being recruited from the interior—for labour purposes—by thousands of young men. That is a fact which cannot be disputed. So that unless we assume that yellow fever is endemic all over the interior, we must conclude that these towns are being regularly recruited with thousands of non-immunes. Now, if, as appears to be the case, the non-immune negroes suffer as much from yellow fever as Europeans, and in equally severe form—if yellow fever exists constantly among the resident native population in a mild form, I find it difficult to believe that its presence would not have been noted in the form of occasional epidemics. Of course, it will be said that the European doctor does not come much in contact with the native population and its diseases. That is true to some extent; but two points must be remembered, namely, that every special outbreak of disease among natives is immediately reflected in the hospital, and, second, that in the event of deaths the bodies have to be buried, so that it is difficult for me to conceive that any such outbreak of severe yellow fever among negro non-immunes, such as ought to occur if endemic yellow fever were prevalent to any extent, could possibly be overlooked. I know that when I was port medical officer at Freetown, as well as medical officer of health, any special outbreak of disease was at once noted, and consequently I am forced to conclude that such epidemics among the non-immune natives have not occurred.

These two factors, namely, the constant supply of European non-immunes and of negro non-immunes, are, to my mind, two very important points to be borne in mind in considering this question of endemic prevalence of yellow fever in West Africa, and they have not, to my mind, been sufficiently emphasised.

It is impossible, in the time at my disposal, to draw attention to more than a few facts which present themselves to my mind as requiring explanation.

And, first, as to the Gambia outbreak in 1900. At that time yellow fever was reported, on May 18th, as raging at Senegal, about eighty miles distant. On May 23rd, Dr. CHICHESTER recognised a case as occurring in a Syrian. Then cases with deaths occurred as follows:—June 7th, 9th, 12th, 16th, 16th (recovery), 19th, 29th, then a gap and a death on August 6th. The epidemic was checked by removing the European population.

Now, remember that local conditions had not changed for years previously, the European population was the same in composition, the stegomyia was equally prevalent, and yet we have a sudden outbreak of a most virulent and fatal form of yellow fever. And since that time—the conditions *still* remaining the same—a new European population coming in to fill the gaps, we have no recurrence. I must confess that, to my mind, this suggests introduction, not endemicity.

Another point in connection with the endemic prevalence of yellow fever on the West Coast is that Dr. CHICHESTER, who left the Gambia and has since been stationed in Southern and Northern Nigeria, has not recognised similar types of disease elsewhere, though it must be admitted that he was familiar with yellow fever. But he has recognised bilious remittent as being of frequent recurrence, and distinguishes between the two diseases.

Then let us take the Bonny outbreak in 1890-1891, which was undoubtedly yellow fever. There were eleven cases, with nine deaths, out of fifteen white residents, occurring in quite a limited period. Here, again, the conditions had not changed from previous years—same population, same prevalence of stegomyia—and yet yellow fever selects one year to assume a virulent and epidemic form, while no similar epidemic has occurred since. It was attributed at the time to the arrival of a ship from South America; but, whether this is the case or not, the conditions seem again to me to point to introduction.

I should now like to say a word or two about Sierra Leone, with which I was connected for a considerable period. I have already alluded to the influx of non-immunes into Freetown from the Hinterland, and the difficulty of reconciling the absence of epidemic outbreaks among these non-immunes with the endemic prevalence of yellow fever among native residents.

It may be stated here that the resident European population at the census of 1891 was 188, and in 1901, 270.

Among Europeans the last undoubted epidemic was in 1884. That is not in dispute.

But it is suggested that an outbreak occurred in 1894, when there were sixteen European deaths. That is, unfortunately, the year before I went to Sierra Leone, but I have analysed these deaths from the annual report of that year. Of these, one died on his way to Freetown from the Hinterland. Of the remaining fifteen, thirteen were said to be caused by bilious remittent hæmorrhagic fever, and these, it is supposed, *may* have been yellow fever. Of the thirteen, eight only occurred among the residents at Freetown, the others having been brought in to die by steamer or boat, so that the Freetown deaths resolved themselves into eight climatic deaths, spread over a period of twelve months. That cannot by any stretch of imagination be called an epidemic.

Now, from 1895 to 1906, when I was port medical officer of the colony, there was certainly nothing in the nature of an epidemic, and on going over the annual reports I can account for most of the causes of the deaths. In 1895, there were 6 deaths, of which 3 only were due to malarial fever (I only mention malaria, as that is the only disease which is suggested was probably wrongly diagnosed for yellow fever). In 1896, 10 deaths were registered, of which 5 were climatic. In 1897, 15 deaths. Of these, 13 were entered as climatic, but I am unable to find an analysis of them. It is to be noted that the railway work in Freetown had been commenced and was being carried out, and that there was an influx of European railway employees, susceptible both to malaria and yellow fever.

In 1898, 14 deaths—6 remittent fever (of which only 2 were in the town), 2 blackwater.

1899, 9 deaths—2 remittent, 4 blackwater.
1900, 19 ,, 4 malaria, 2 ,,
1901, 10 ,, 5 ,, 3 ,,
1902, 8 ,, 3 ,, 2 ,,
1903, 11 ,, 3 ,, 1 ,,
1904, 12 ,, 4 ,,
1905, 8 ,, 3 ,, 1 ,,

These deaths were spread over the twelve months, so that during these years it will, I think, be admitted that there was nothing in the nature of an epidemic outbreak of any fever, and it must be remembered that

the diagnosis in the majority of these cases from 1899, at any rate, was verified by microscopical examination.

Now it has been suggested that many of the cases of remittent fever were really yellow fever overlooked because the medical officers were not on the look out for the disease.

I find, on going over my annual reports the following facts distinctly stated. In 1897, yellow fever raging in Jamaica. The transport Spartan came over with troops and was placed in quarantine. In 1898, yellow fever still in Jamaica. Spartan arrived with more troops. No local outbreak. In 1899, yellow fever reported as raging with great severity on Ivory Coast. Heavy mortality. No local outbreak. 1900, yellow fever notified in Senegal and in Gambia. No local outbreak. 1901, isolated cases reported throughout the year from Senegal. 1903, yellow fever reported from Ivory Coast. 1904, yellow fever at Grand Bassam, Ivory Coast. In the same year, in my lectures on tropical hygiene (which were published), I specially drew attention to the prevalence of yellow fever on different parts of the West Coast, to the fact that stegomyia was the most common mosquito in Freetown, and pointed out the danger of its introduction.

So that I think I may safely say that, so far as I was concerned, the existence of yellow fever and the possibility of its introduction to Sierra Leone was constantly before me. And yet the figures I have given will show that there was nothing to indicate its presence in the form which it assumed elsewhere, and which we might be certain it would assume in Freetown in a population of over 200 Europeans, many of whom were non-immunes, and living, remember, in the midst of the natives.

So that my personal experience forces me to conclude that yellow fever did not exist among Europeans at any rate during these 12 yeass.

I certainly agree with Dr. TULLOCH that there is a distinct clinical entity which may conveniently be described as bilious remittent fever, and which is simply a form of that protean disease malaria. It is characterised by an irregular temperature, the presence of malignant sub-tertian parasites in the peripheral circulation, though these are often rare and difficult to detect when quinine has been taken, marked gastro-hepatic symptoms, a variable degree of jaundice, absence of albumen in the urine, amenability to the influence of quinine, and a pulse rate which varies in proportion to the temperature. So much did the last

feature strike me that in lecturing to the students at Liverpool on the malignant fevers of the West Coast, I have pointed out, that with a little experience, one could tell the temperature from the pulse rate. And that particular fact, having impressed itself upon me as being a marked feature of the malignant fevers of the West Coast, I can hardly believe that I should have overlooked any marked departures from it. I do not deny that I have met with cases of high temperature, with a slow pulse rate, and cases which have been resistent to quinine, but these have been occasional, and other features have not been sufficient to justify a diagnosis of yellow fever. So that my experience, at any rate, leads me still to believe that malignant malarial fevers have been the most common type on the West Coast, although the presence of yellow fever has always to be kept in mind. And as for reluctance to make a diagnosis for yellow fever, speaking for myself, and I am sure the greater proportion of the staff will agree with me, I should have had no hesitation whatever in drawing the attention of the executive to it, whatever their action might subsequently have been.

On the whole, the evidence seems to me to point to the introduction of yellow fever into definite localities, to its epidemic prevalence, and then to its disappearance, to reappear in some neighbouring or distant spot, to which it might have been carried by the many and frequent means of transport which now exist.

But, on the other hand, there are many points in favour of Professor Boyce's theory, and one of the strongest to my mind is: Why do the resident natives of a locality not suffer from yellow fever when an epidemic was raging among Europeans, if they are not immune? I must confess this is very difficult to get over, unless we drag in racial immunity.

We are indebted to Sir Rubert Boyce for drawing attention to this subject—we are all searchers after truth, and however opinions may differ, discussion can only be productive of good. I certainly consider that Professor Boyce has made out a strong case for a commission of enquiry, and I hope it will not be long before this is given effect to. In the meantime, one thing we are all agreed upon, and that is the absolute necessity for vigorous anti-stegomyia measures, in the presence of yellow fever on any part of the West Coast, and it is a matter of much satisfaction to me that the Executive has had this very effective stimulus to

action. But one word of warning. There is a danger that in warring with the stegomyia, the wily anopheles will be lost sight of, and malaria still claims its numerous victims on the Coast.

Fleet-Surgeon COLLINGWOOD [letter read by the Secretary in his absence]: I regret very much I cannot be present at the meeting, for the subject is one of great interest to me, as I was on the West Coast in 1894 and 1895, in a gunboat, and had a great deal of fever on board returned as malarial, though in some cases with severe vomiting. I was puzzled as to diagnosis. In these, quinine did no good; but, though some cases were critical, they all recovered. I see now there was yellow fever on the Coast in 1894, though I did not hear anything about it at the time, only that fever was rather bad ashore and the road to the cemetery at Sierra Leone was frequently used.

Why some medical officers find quinine no good as a prophylactic and others swear by it, may depend upon the fact that subsequent cases were mild yellow fever and not malaria, a point of some interest. As regards mistaken diagnosis, this would apply to both sides in the discussion on yellow fever on the West Coast, especially in past ages, and even at present there is no absolute scientific test of the diagnosis of yellow fever, so the truth may lie much where it did before this discussion arose.

Captain SKELTON: In view of the great difficulty that appears to exist in deciding from symptoms whether a case is bilious remittent or mild yellow fever, I should like to ask whether any work on the fixation of complement has been carried out by Sir RUBERT BOYCE or any of his co-workers in connection with these diseases. An analagous instance of the serum method of diagnosis lies in the precipitin reaction that can be brought about in the case of hydatid fluid in the presence of its specific antigen, and this reaction has been found to be of value as an aid to diagnosis. The difficulty in yellow fever lies in obtaining a suitable antigen. I would suggest that when this difficulty arises it might be got over by obtaining an extract from, say, the liver of an undoubted and proved case of yellow fever from Central America. This method might be made some use of by skilled observers.

As to the alleged reluctance on the part of medical men to diagnose yellow fever, my experience does not bear this out. I have had the good

fortune to serve on the West Coast of Africa. Bilious remittent fever cases are admitted into the military hospital in Sierra Leone fairly frequently. In this district at least one has no axe to grind or outside influences to pay regard to. If the opinion has been arrived at that our military bilious remittent fever cases were really mild yellow fever, yellow fever would certainly have been diagnosed—at any rate until the contrary was clearly established.

Dr. W. CARNEGIE BROWN: There can be no better evidence of the interest which the Society takes in the epidemiology of yellow fever, and the importance which it attaches to the whole subject, than the fact that since our formation, less than four years ago, this is the fifth occasion on which it has been before us, and the third full evening which we have devoted to the discussion of Sir RUBERT BOYCE'S paper. Now although I have, unless unwittingly, no personal experience of yellow fever, I have seen in the Eastern Tropics a great deal of the disorder which is popularly termed bilious remittent fever; and I venture to speak, because in making a distinction between the two diseases, the clinical features of the one are just as important as those of the other, and it is essential to a logical discussion of the question that these should be clearly defined and thoroughly understood. Now, so far, we have had no exact definition of what bilious remittent fever really is. In the excellent and graphic description which Dr. TULLOCH gave of the symptoms, he said that in Trinidad it was considered to be a form of malaria, and in this he follows what is the generally accepted opinion. In most, if not all of the text books, it is described as a clinical variety of subtertian malarial infection. Well, if that is the case, there should be no difficulty about diagnosis. In the great majority of cases the organisms ought to be easily demonstrable, and an effective answer would be given to Sir RUBERT BOYCE'S contention by showing that in the cases which he claims to be yellow fever, malignant malarial parasites are present, and that, after a few days, crescents make their appearance. I have not heard, however, that anyone has done this. I find, too, that in my own practice in the years 1898-1904, I have records of 87 cases which were diagnosed as bilious remittent fever, and which presented all the characteristics of that distressing complaint, and that in only six of them were parasites found. In four there were ring forms, and in two the

parasite was pigmented and malignant. In two crescents were found. No doubt these were cases of genuine malaria, but what about the others?

My experience also differs from that of Dr. TULLOCH, in that in this variety of case I have not found quinine possess the slightest efficacy unless parasites were found, and that is, I think, a common eastern experience. Further, it is not a fever of a malignant type, and, although it may be protracted, it is very rarely fatal.

What then is the bilious remittent fever of the East? It is true that it is generally a town disease, and that in the towns stegomyia is everywhere abundant. But it does not show, and never has shown, any tendency to become epidemic, and for many reasons it is impossible to believe that it is even a modified form of yellow fever. Indeed, many of these cases may be included in the syndrome which ROGERS differentiates as seven-day fever. The symptoms are practically identical with those described by him and in which he also finds quinine valueless; but the term which he has chosen is inappropriate, and, I venture to think, unfortunate. There is no limitation to seven days or to two remissions. In Malaya, in my experience, the usual duration was twelve to fifteen days, and there were often four, five or more remissions. It is possible, and I think probable, that a pathogenic organism as yet unknown may be found in these cases, and that this disorder will ultimately be clearly and scientifically separated from a host of other fevers with which it is at present confused; but in the meantime it is open to Sir RUBERT BOYCE to say to those who believe that bilious remittent fever is always a malarial infection, that at least one elementary clinical distinction between malaria and all other diseases has been omitted.

The PRESIDENT (Professor RONALD ROSS): Fortunately it is the privilege of a President to efface himself as much as possible in discussions like these, but I have a point or two of some importance on which I should like to ask for information. Does anyone present know the leucocytic formula in yellow fever? I think Dr. DURHAM did some leucocytic counting in yellow fever. It is an important point. There is a very definite leucocytic count in a closely allied disease, dengue.

Dr. G. C. LOW: There is nothing specially characteristic either in the absolute or differential counts. Dr. ST. GEORGE GRAY and I have made

several counts on yellow fever in St. Lucia in 1901. The total number of leucocytes was generally about normal or perhaps slightly decreased, leucocytosis was not noted, and in the differential count there was no large mononuclear increase of equivalent proportions to that seen in malaria. Three cases counted by Dr. GRAY gave respectively for the large mononuclears, 11, 7·66 and 8·33 per cent. Major GORGAS—replying to Dr. GRAY, who wrote him on this subject—also stated that an increase in the number of large mononuclear leucocytes had not as yet been observed in cases of yellow fever. A count sent to Dr. GRAY and myself by Dr. GUITERAS from Havana also showed the leucocytes normal in number with no large mononuclear increase.

This absence of a relative increase of the large mononuclear leucocytes with a total leucocyte count about normal, Dr. GRAY had thought might have been useful in diagnosing between malaria and yellow fever. I think that it would be of only very limited value, as in some cases of primary malaria, especially in the initial stage, the increase is not noticeable, and therefore to base any conclusions on this might lead to serious error.

Dr. SEIDELIN: I have made a good many blood examinations and and also a number of leucocyte counts, and I have always found slight leucopenia, and at no stage in the disease was there any leuocytosis. In my experience there seemed to be some increase in number of mononuclears, but the percentage was not nearly so high as in malaria. Eosinophilia I have never found; in fact, the eosinophiles were present in less than one per cent. of the total number of leucocytes. I may also add that the blood-platelets are very numerous, besides much larger than in normal blood and in other diseases, as malaria and other parasitical diseases, in which I have made blood examinations.

The PRESIDENT: Can anyone tell us whether yellow fever cases have occurred inland—in the Hinterland, for example—without a previous outbreak being noted nearer the Coast? That is, must the outbreak always start from the Coast and travel inland?

Sir RUBERT BOYCE: BOUFFARD LE DANTEC and others describe yellow fever outbreaks in the Upper Senegal and Niger Territory

reaching into the Soudan, and Augustin, in his curious works on yellow fever, 1910, records many epidemics in the Soudan, but unless his sources of information are verified, his statement must be taken with great reserve.

The PRESIDENT: Another point is why should endemic yellow fever be limited to West Africa? Stegomyia, we know, extends right across Africa; therefore, why does not the disease occur on the other side of the continent? The problem would seem to be to discover the reason why yellow fever does not occur over the whole tropical world. Of course the supposition is that it does occur all over the world, but in a different form to what is described in America. I should doubt whether we get typical yellow fever in India.

Sir RUBERT BOYCE then made the following reply (in the course of which he shewed some lantern slides to illustrate the morbid anatomy of the liver and kidneys in yellow fever, as well as some stastistical diagrams): One of the speakers this evening has raised a very important query, Why we have not got pathological specimens of bilious remittent fever? Now, if any member coming across a case would only send specimens of organs, we might have a chance of comparing the lesions of genuine bilious remittent fever with those of yellow fever. It would be something well worth doing.

I am showing to-night sections of the liver and kidneys of two cases of yellow fever occurring in black men, which proved fatal this year in West Africa; one of these a private in the West India Regiment in Tower Hill Barracks, the other of a native of Sekondi, and they show that yellow fever does occur in blacks. Some deny this, but the majority of medical men now recognise that it can occur in the black, although it is rare. All authorities have drawn attention to this fact, and you will see that HIRSCH quotes fifteen authorities for the statement.

It is a curious fact, if true, that Chinamen do not get yellow fever. I have not been able to corroborate the statement, but if any gentleman knows of cases in Chinamen, it would be very interesting. DANIEL BLAIR, I believe, describes cases in Chinamen in his time in Demarara. It would, however, be very interesting to know whether Chinamen get

yellow fever or not. We know they get bilious remittent fever. Do they escape yellow fever?

[Dr. SEIDELIN replied: I may state that I have seen several cases of yellow fever in Chinamen, as I have also seen at least one in a negro.]

I wish to make my reply on the discussion as short as possible, and I will commence my remarks as follows:—

1. I have demonstrated the almost continuous occurrence of well-recognised outbreaks of yellow fever for the past hundred years, and an unbroken record for the last twenty years.

These are facts which have not been disputed. The figures which I have given have been collected from authentic sources, and have been verified in every case. I have searched authorities and have interviewed men on the spot, and have in doubtful cases examined the clinical histories and post-mortem records. It is only on data of this kind that I have made the statement.

2. The fact that the natives of West Africa have shown an almost inappreciable death-rate from the disease, whereas amongst the whites the death-rate has varied from 70 per cent. to 100 per cent.

In fact, there is almost immunity amongst natives—almost complete absence of deaths compared to the death-rate amongst the whites. If you compare the table that I have made of the death-rate amongst whites and blacks on the coast of Africa with the death-rate amongst the Indo-Spanish natives of Rio de Janeiro or Vera Cruz, you will find the proportion very nearly the same.

3. That with the immense black population and the very scanty white population, the death-rate amongst the whites has been vastly greater than amongst the natives, and that this extraordinary difference can only be explained on the hypothesis of immunity.

I cannot see how any other hypothesis will explain why the natives in Africa—in Freetown and Sekondi, for example—escaped with only four fatal cases of yellow fever in 1910, and that, too, in spite of their numerical preponderance. The natives must have been exposed to infection all round, as there was no attempt at isolation in their case. What protected them? Give me some other reasonable hypothesis and I will take it to-morrow. I have heard Sir PATRICK MANSON's theory as to why this immense native population goes free from yellow fever; the

fact, however, remains that in 1910 Europeans living amongst natives got yellow fever, and that his theory will not explain the facts, and is one, moreover, which is not accepted. I maintain that nothing will explain these facts so well as the hypothesis of acquired immunity, maintained by OTTO and MEUMANN, and by many other investigators which the French, Brazilian and American Governments have sent to yellow fever countries. These observers can only find this one hypothesis of immunity as adequate to explain the comparative immunity of the native as against the white newcomer.

4. The fact that the West Indian negroes are susceptible, just like the white newcomers.

5. The fact that the natives can get yellow fever, in other words that the black has no natural immunity against yellow fever.

6. That the explanation of the existence of yellow fever in West Africa is similar to that adopted for other countries by the Brazilian, Cuban, French, German and American Official Commissions, *e.g.*, that the natives or indigenous races are the source of the virus, and that, as in malaria, they harbour the disease in some form, and,

7. That segregation affords absolute protection against the disease.

I have stated that if a European resided in the native towns of Sekondi or Freetown in 1910, and slept there, he in all probability got yellow fever. On the other hand, that if he remained isolated on the hillside station or in the barracks at Freetown, or at some distance from the native town of Sekondi, he remained free from yellow fever. This showed there was infection at any rate in the native towns, and that segregation afforded complete protection. The moment a man took up his residence in the town he became liable to infection. I cannot conceive any other explanation that can adequately account for this fact except the doctrine of immunity, as now generally accepted in connection with yellow fever.

I did not say that yellow fever was necessarily endemic in every village and town in West Africa, but that it was endemic in West Africa precisely as it is endemic in Venezuela, or along the Amazon ; that is to say, that the disease was smouldering amongst the natives in very many centres, but not necessarily in all centres ; precisely as in the case of diseases

like cholera, which may be endemic in a country, but not necessarily present in every village or town.

I desire to guard against the statement being made that we are afraid to acknowledge that yellow fever is present in West Africa. A great authority upon yellow fever BERENGER FERAUD, has plainly stated that the English in West Africa appear to have an invincible reluctance to acknowledge the existence of yellow fever in their colonies. If there has been this reluctance, it is now our duty to acknowledge the presence of this pest and get rid of it.

ELECTION OF MEMBERS.

The following were duly elected as Fellows of the Society :—

Captain F. E. Bissell, R.A.M.C., Northern Nigeria.

E. Nicolas Darwent, M.D., Trinidad.

R. F. de Boissiere, L.R.C.P., Kadavu, Fiji.

Professor José de Magalhães, Lisbon.

William J. Dixon, M.B., M.R.C.S., Taiping, F.M.S.

E. Frederic Eliot, F.R.C.S.E., Menaggio.

Frederick Hall, L.R.C.P., Mokogai, Fiji.

Seale Harris, M.D., Mobile, Alabama.

Fred. L. Henderson, M.R.C.S., East African Protectorate.

H. N. H. Joynt, M.D., Lautoka, Fiji.

W. F. M. Loughman, L.R.C.S., Bareilly, India.

O. E. Maguire, M.D., Nukualofa, Tonga.

Fitzmaurice Manning, S.M.O., N. Nigeria.

Annibal C. Merides, Director Bact. Lab., Angola.

Thomas H. Ravenhill, M.B., Antofogasta.

Lieut.-Col. W. S. Ricketts, I.M.S. (Ret.), Naples.

Lieut.-Col. James R. Roberts, F.R.C.S., Indore.

B. van Someren Taylor, M.D., Foochow.

John P. Tulloch, M.D., Chaguanas, Trinidad.

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